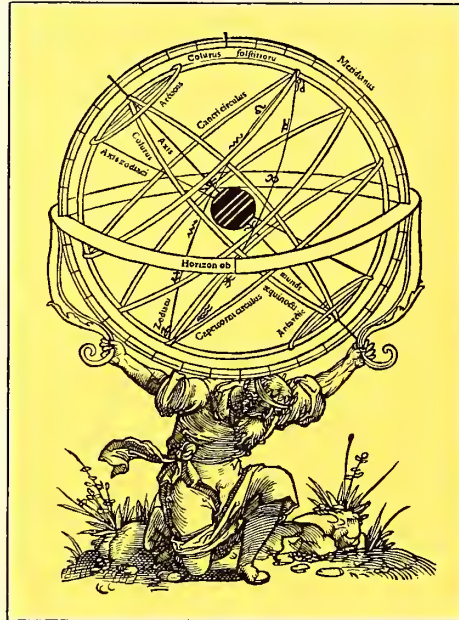




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# PHILOSOPHICAL TRANSACTIONS.

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I. *Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, Esq.,  
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§. 18. *On Induction.* ¶ i. *Induction an action of contiguous particles.*  
¶ ii. *Absolute charge of matter.* ¶ iii. *Electrometer and inductive  
apparatus employed.* ¶ iv. *Induction in curved lines.* ¶ v. *Specific  
inductive capacity.* ¶ vi. *General results as to induction.*

¶ i. *Induction an action of contiguous particles.*

1161. THE science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its over-ruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels, or even equals in importance that called *Induction*. It is of the most general influence in electrical phenomena, appearing to be concerned in every one of them, and has in reality the

character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phenomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions, upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state, though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action, and the possible subjugation of many dissimilar phenomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquified, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement* of the particles consistent with the transfer of the elements of the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing actions do not interfere; and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into



a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of EPINUS, CAVENDISH, POISSON, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951. 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances; and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view, not merely in the accordance of known facts with it, but in the consequences which would flow from it if true; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which, according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164. 949.), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependence of induction on the particles; and though this, in the theory of POISSON and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever present question on my mind has been, whether electricity has

an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attempts, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connection with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting, be charged with one electric force independently of the other, in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of COULOMB upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces\*, are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electrics or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and, being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass

\* Memoires de l'Academie, 1786, pp. 67. 69. 72; 1787, p. 452.



vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connection with the wires, and supplied in every direction with bands of tin foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube above six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290.) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation), and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171.), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least signs of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the sup-

position of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the other, and yet possible to have cases in which an isolated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids: these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.) The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, &c. &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negative result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only relatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All phenomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phenomena of electricity.

### ¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly, as to leave no doubt of the results behind.



1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of COULOMB, constructed, generally, according to his instructions\*, but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. RITCHIE†. It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions or  $1440^\circ$ , and yet, when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0.3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to COULOMB's direction, the arm carrying the ball being 2.4 inches long, and the other only 1.2 inches: to this was attached the vane, also described by COULOMB, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it, at the distance of 0.4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to COULOMB's method: the stem was fixed at its upper end in a block or vice, supported on three short feet; and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a

\* Memoires de l'Academie, 1785, p. 570.

† Philosophical Transactions, 1830.



conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When, therefore, the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of  $90^\circ$ ; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusting screws on which the whole instrument stands, the glass torsion thread could be brought accurately into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of  $90^\circ$  was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in COULOMB's original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181.) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to  $0^\circ$  on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at  $30^\circ$ , and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring  $0^\circ$  upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of  $30^\circ$  being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the

bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of  $30^\circ$  between the centres of the balls was adopted, and found abundantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible by diminishing this distance was at perfect command; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The COULOMB balance electrometer requires experience to be understood; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of  $400^\circ$  at the standard distance of  $30^\circ$ , it took nearly four hours to sink to  $50^\circ$  at the same distance; the average loss from  $400^\circ$  to  $300^\circ$  being at the rate of  $2^\circ\cdot7$  per minute, from  $300^\circ$  to  $200^\circ$  of  $1^\circ\cdot7$  per minute, from  $200^\circ$  to  $100^\circ$  of  $1^\circ\cdot3$  per minute, and from  $100^\circ$  to  $50^\circ$  of  $0^\circ\cdot87$  per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducing surfaces of the conductors should have a constant form and state, and be at a constant distance from each other; and that either solids, fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it; the interval between the two being the space through which the induction was to take place. A section of it is given (Plate I. fig. 1.) on a scale of one half: *a, a* are the two halves of a brass sphere, with an air-tight joint at *b*, like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d*, which is itself attached either to the metallic foot *e*, or to an air pump. The aperture within the hemisphere at *f* is very small: *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball



and its stem passes;  $h$  is the inner ball, also of brass; it screws on to a brass stem  $i$ , terminated above by a brass ball  $B$ ;  $l$  is a mass of shell lac, moulded carefully on to  $i$ , and serving both to support and insulate it and its balls  $h$ ,  $B$ . The shell-lac stem  $l$  is fitted into the socket  $g$ , by a little ordinary resinous cement, more fusible than shell-lac, applied at  $m$  in such a way as to give sufficient strength and render the apparatus air-tight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball  $h$  and the surrounding sphere  $a$ ,  $a$ . The ball  $h$  has a small aperture at  $n$ , so that when the apparatus is exhausted of one gas and filled with another, the ball  $h$  may itself also be exhausted and filled, that no variation of the gas in the interval  $o$  may occur during the course of an experiment.

1189. It will be unnecessary to give the dimensions of all the parts, since the drawing is to a scale of one half: the inner ball has a diameter of 2.33 inches, and the surrounding sphere an internal diameter of 3.57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0.62 of an inch; and the extent of this place or plate, i. e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball  $h$  and sphere  $a$ ,  $a$ ; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire  $i$ , so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by scale in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar  $g$  was made as thin as could be, that the lac might be as large there as possible. In order that at every re-attachment of the stem to the upper hemisphere the ball  $h$  might have the same relative position, a gauge  $p$  (fig. 2.) was made of wood, and this being applied to the ball and hemisphere whilst the cement at  $m$  was still soft, the bearings of the ball at  $q$ ,  $q$ , and the hemisphere at  $r$ ,  $r$ , were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186.), i. e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top  $k$  of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then



in falling from  $600^{\circ}$  to  $400^{\circ}$  the average loss was  $8^{\circ}6$  per minute; from  $400^{\circ}$  to  $300^{\circ}$  the average loss was  $2^{\circ}6$  per minute; from  $300^{\circ}$  to  $200^{\circ}$  it was  $1^{\circ}7$  per minute; from  $200^{\circ}$  to  $170^{\circ}$  it was  $1^{\circ}$  per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above  $212^{\circ}$ , air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *h* and *B*, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178.). Now though the largest portion of this induction is between the ball *h* and the surrounding sphere *a*, yet the wire *i* and the ball *B* determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that respect remain the same, whilst the medium within at *o o*, may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to

prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184.), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one-sixteenth or one-twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of  $30^\circ$  by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls*.

1198. One of the inductive apparatus, as, for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k*, fig. 1.), then introduced into the electrometer, and the repulsive force (at the distance of  $30^\circ$ ) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics*.

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation

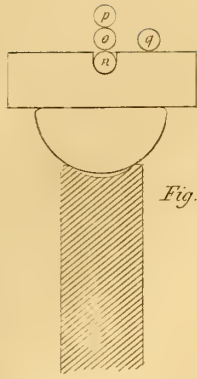


Fig. 9.

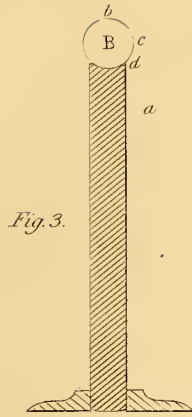


Fig. 3.

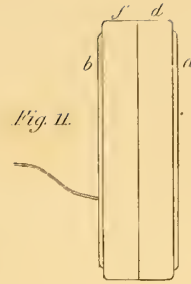


Fig. 11.

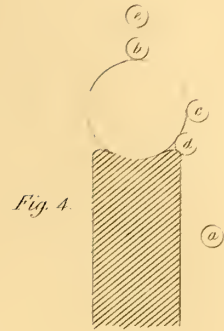


Fig. 4.



Fig. 5.

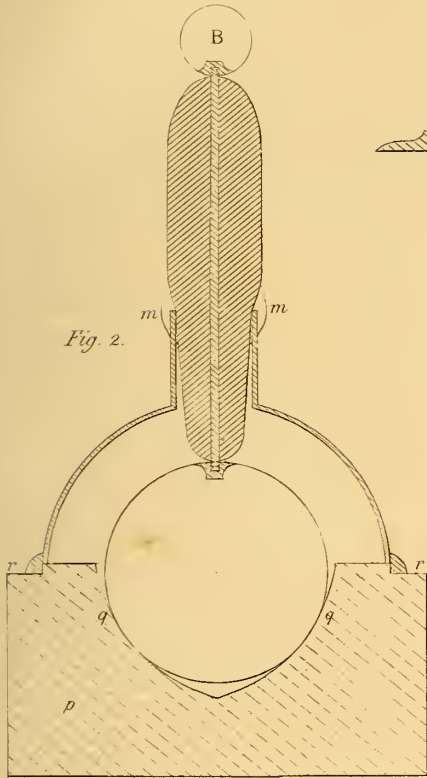


Fig. 2.

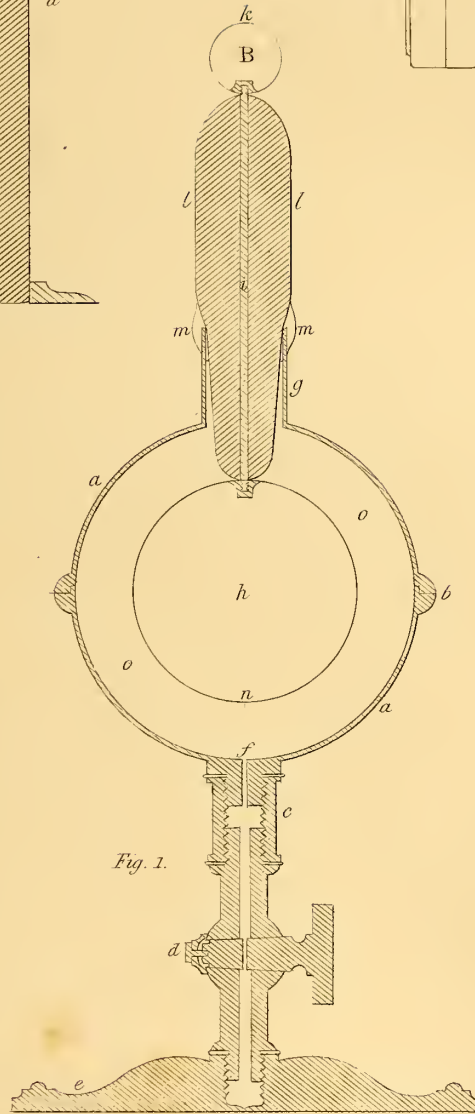


Fig. 1.

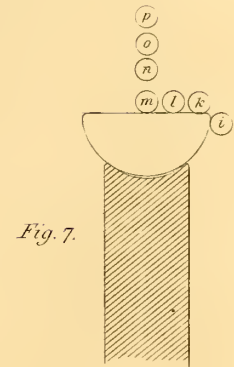


Fig. 7.

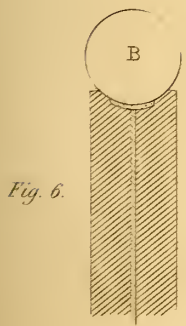


Fig. 6.

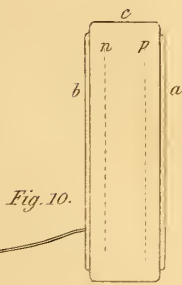


Fig. 10.

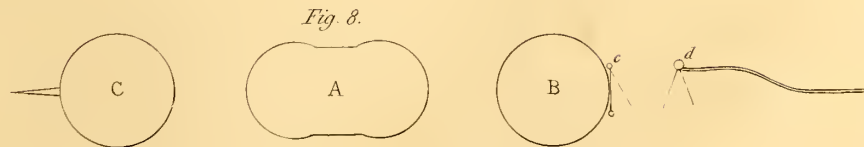


Fig. 8.





purposely, to the amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232.). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated*: the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experiments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a per-

fect and instantaneous discharge, giving then no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

App. i.	App. ii.
Balls 160°	
. . . .	0°
254° . . . .	
250 . . . .	
divided and instantly taken	
. . . .	122
124 . . . .	
1 . . . .	after being discharged.
. . . .	2 after being discharged.

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischageable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after



the division was found with  $122^\circ$ , this amount *at least* may be taken as what it had received. On the other hand  $124^\circ$  minus  $1^\circ$ , or  $123^\circ$ , may be taken as the half of the transferable charge retained by app. i. Now these do not differ much from each other, or from  $124^\circ \cdot 5$ , the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between  $254^\circ$  and  $250^\circ$  of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

App. i.	App. ii.
Balls $150^\circ$	
. . . .	$152^\circ$
. . . .	148
divided and instantly taken	
$70^\circ$ . . . .	
. . . .	78
. . . .	5 immediately after discharge.
0 . . . . .	immediately after discharge.

1211. The transferable charge being  $148^\circ - 5^\circ$ , its half is  $71^\circ \cdot 5$ , which is not far removed from  $70^\circ$ , the half charge of i.; or from  $73^\circ$ , the half charge of ii.: these half charges again making up the sum of  $143^\circ$ , or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from  $0\cdot62$  to  $0\cdot435$  of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of  $469^\circ$  was divided with app. i., the former retained a charge of  $225^\circ$ , whilst the latter showed one of  $227^\circ$ , i. e. the former had lost  $244^\circ$  in communicating  $227^\circ$  to the latter: on the other hand, when app. i. had a transferable charge in it of  $381^\circ$  divided by contact with app. ii., it lost  $181^\circ$  only, whilst it gave to app. ii. as many as  $194^\circ$ :—the sum of the divided forces being in the first instance

*less*, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

#### ¶ iv. *Induction in curved lines.*

1215. Amongst those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge, (though they have long been to me indications of prior molecular action (1230.)) I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines. It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0·9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and COULOMB'S electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360°, and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated\*, separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (figs. 3. and 4.) observed in succession, was as follows:

\* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places that would have given to it, if uninsulated, a different condition.

at <i>a</i>	. . .	above 1000°
<i>b</i> it was	. . .	149
<i>c</i>	. . . . .	270
<i>d</i>	. . . . .	512
<i>b</i>	. . . . .	130

1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell-lac cylinder; for whatever electricity the ball B received by *communication* from the shell-lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as might be expected from an inductive action in straight lines, but that obtained at *b* is *not so*: it is clearly a charge by induction, but *induction in a curved line*; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from B, could not, in consequence of the size of B, be connected by a straight line with any part of the excited and inducing shell-lac.

1220. To suppose that the upper part of the *uninsulated* ball B, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.); and though some persons may not be prepared as yet to admit this with respect to insulated conductors, all will as regards uninsulated conductors like the ball B; and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with B, uninsulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state; it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of B.

1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball B a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface: but if the plate be enlarged to about  $1\frac{1}{2}$  or 2 inches in diameter, C (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even *above the middle* at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball B (fig. 6.)



so as to keep it in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phenomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.

1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was 1.36 of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was  $112^\circ$ , at *k*  $108^\circ$ , at *l*  $65^\circ$ , at *m*  $35^\circ$ ; the inductive force gradually diminishing, as might have been expected, to this point. But on raising the carrier to the position *n* the charge increased to  $87^\circ$ ; and on raising it still higher to *a*, the charge still further increased to  $105^\circ$ : at a higher point still, *p*, the charge taken was smaller in amount, being  $98^\circ$ , and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilinear form by the shape, position, and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another:—all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.

1225. As another proof that the whole of these actions were inductive I may state a result which was exactly what might be expected, namely, that if uninsulated conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phenomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined

towards them that the lateral tension or mutual repulsion of the lines of force before spoken of, (1224.) by which their inflexion is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions  $k, l, m, n, o, p$ , (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substitution of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac, (1272.) and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (1224.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0.6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at  $n, o, p$ , and  $q$ . Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received. This was done by breathing and wiping (1203.), and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same: and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negated.

1229. In these experiments with solid dielectrics, the degree of charge assumed by the carrier ball at the situations  $n, o, p$  (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B, or even a plate, provided the edges be not too thin, be held before it, a small electrometer at  $c$  or at  $d$ , uninsu-



lated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by NICHOLSON\*, who, however, reasons erroneously upon it. As to its introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its static state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductive and the inductive surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

\* Encyclopædia Britannica, vol. vi. p. 504.



1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

1234. One of the inductive apparatus already described (1187, &c.) had a hemispherical cup of shell-lac introduced, which being in the interval between the inner ball and the lower hemisphere, nearly occupied the space there; consequently when the apparatus was charged, the lac was the dielectric or insulating medium through which the induction took place in that part. When this apparatus was first charged with electricity (1198.) up to a certain intensity, as  $400^{\circ}$ , measured by the COULOMB'S electrometer (1180.), it sank much faster from that degree than if it had been previously charged to a higher point, and had gradually fallen to  $400^{\circ}$ ; or than it would do if the charge were, by a second application, raised up again to  $400^{\circ}$ ; all other things remaining the same. Again, if after having been charged for some time, as fifteen or twenty minutes, it was suddenly and perfectly discharged, even the stem having all electricity removed from it (1203.), then the apparatus being left to itself, would gradually recover a charge, which in nine or ten minutes would rise up to  $50^{\circ}$  or  $60^{\circ}$ , and in one instance to  $80^{\circ}$ .

1235. The electricity, which in these cases returned from an apparently latent to a sensible state, was always of the same kind as that which had been given by the charge. The return took place at both the inducing surfaces; for if after the perfect discharge of the apparatus the whole was insulated, as the inner ball resumed a positive state the outer sphere acquired a negative condition.

1236. This effect was at once distinguished from that produced by the excited stem acting in curved lines of induction (1203. 1232.), by the circumstance that all the returned electricity could be perfectly and instantly discharged. It appeared to depend upon the shell-lac within, and to be, in some way, due to electricity evolved from it in consequence of a previous condition into which it had been brought by the charge of the metallic coatings or balls.

1237. To examine this state more accurately, the apparatus, with the hemispherical cup of shell-lac in it, was charged for about forty-five minutes to above  $600^{\circ}$  with positive electricity at the balls *h* and *B* (fig. 1.) above and within. It was then discharged, opened, the shell-lac taken out, and its state examined; this was done by bringing the carrier ball near the shell-lac, uninsulating it, insulating it, and then observing what charge it had acquired. As it would be a charge by induction, the state of the ball would indicate the opposite state of electricity in that surface of the shell-lac which had produced it. At first the lac appeared quite free from any charge; but gradually its two surfaces assumed opposite states of electricity, the concave surface, which had been next the inner and positive ball, assuming a positive state, and the convex surface, which had been in contact with the negative

coating, acquiring a negative state; these states gradually increasing in intensity for some time.

1238. As the return action was evidently greatest instantly after the discharge, I again put the apparatus together, and charged it for fifteen minutes as before, the inner ball positively. I then discharged it, instantly removing the upper hemisphere with the interior ball, and, leaving the shell-lac cup in the lower uninsulated hemisphere, examined its inner surface by the carrier ball as before (1237.). In this way I found the surface of the shell-lac actually *negative*, or in the reverse state to the ball which had been in it; this state quickly disappeared, and was succeeded by a positive condition, gradually increasing in intensity for some time, in the same manner as before. This first negative condition of the surface opposite the positive charging ball is a natural consequence of the state of things, the charging ball being in contact with the shell-lac only in a few points. It does not interfere with the general result and peculiar state now under consideration, except that it assists in illustrating in a very marked manner the ultimate assumption by the surfaces of the shell-lac of an electrified condition, similar to that of the metallic surfaces opposed to or against them.

1239. *Glass* was then examined with respect to its power of assuming this peculiar state. I had a thick flint glass hemispherical cup formed, which would fit easily into the space *o* of the lower hemisphere (1188. 1189.); it had been heated and varnished with a solution of shell-lac in alcohol, for the purpose of destroying the conducting power of the vitreous surface. Being then well warmed and experimented with, I found it could also assume the *same state*, but not apparently to the same degree, the return action amounting in different cases to quantities from  $6^{\circ}$  to  $18^{\circ}$ .

1240. *Spermaceti* experimented with in the same manner gave striking results. When the original charge had been sustained for fifteen or twenty minutes at about  $500^{\circ}$ , the return charge was equal to  $95^{\circ}$  or  $100^{\circ}$ , and was about fourteen minutes arriving at the maximum effect. A charge continued for not more than two or three seconds was here succeeded by a return charge of  $50^{\circ}$  or  $60^{\circ}$ . The observations formerly made (1234.) held good with this substance. *Spermaceti*, though it will insulate a low charge for some time, is a better conductor than shell-lac, glass, and sulphur; and this conducting power is connected with its readiness in exhibiting the particular effect under consideration.

1241. *Sulphur*.—I was anxious to obtain the amount of effect with this substance, first, because it is an excellent insulator, and in that respect would illustrate the relation of the effect to the degree of conducting power possessed by the dielectric (1247.); and in the next place, that I might obtain that body giving the smallest degree of the effect now under consideration, for the investigation of the question of specific inductive capacity (1277.).

1242. With a good hemispherical cup of sulphur cast solid and sound, I obtained the return charge, but only to an amount of  $17^{\circ}$  or  $18^{\circ}$ . Thus glass and sulphur,



which are bodily very bad conductors of electricity, and indeed almost perfect insulators, gave very little of this return charge.

1243. I tried the same experiment having *air* only in the inductive apparatus. After a continued high charge for some time I could obtain a little effect of return action, but it was ultimately traced to the shell-lac of the stem.

1244. I sought to produce something like this state with one electric power and without induction; for upon the theory of an electric fluid or fluids, that did not seem impossible, and then I should have obtained an absolute charge (1169. 1177.), or something equivalent to it. In this I could not succeed. I excited the outside of a cylinder of shell-lac very highly for some time, and then quickly discharging it (1203.), waited and watched whether any return charge would appear, but such was not the case. This is another fact in favour of the inseparability of the two electric forces, and another argument for the view that induction and its concomitant phenomena depend upon a polarity of the particles of matter.

1245. Although inclined at first to refer these effects to a peculiar masked condition of a certain portion of the forces, I think I have since correctly traced them to known principles of electrical action. The effects appear to be due to an actual penetration of the charge to some distance within the electric, at each of its two surfaces, by what we call conduction; so that, to use the ordinary phrase, the electric forces sustaining the induction are not upon the metallic surfaces only, but upon and within the dielectric also, extending to a greater or smaller depth from the metal linings. Let *c* (fig. 10.) be the section of a plate of any dielectric, *a* and *b* being the metallic coatings; let *b* be uninsulated, and *a* be charged positively; after ten or fifteen minutes, if *a* and *b* be discharged, insulated, and immediately examined, no electricity will appear in them; but in a short time, upon a second examination, they will appear charged in the same way, though not to the same degree, as they were at first. Now suppose that a portion of the positive force has, under the coercing influence of all the forces concerned, penetrated the dielectric and taken up its place at the line *p*, a corresponding portion of the negative force having also assumed its position at the line *n*; that in fact the electric at these two parts has become charged positive and negative; then it is clear that the induction of these two forces will be much greater one towards the other, and less in an external direction, now that they are at the small distance *n p* from each other, than when they were at the larger interval *a b*. Then let *a* and *b* be discharged; the discharge destroys or neutralizes all external induction, and the coatings are therefore found by the carrier ball unelectrified; but it also removes almost the whole of the forces by which the electric charge was driven into the dielectric, and though probably a part goes forward in its passage and terminates in what we call discharge, the greater portion returns on its course to the surfaces of *c*, and consequently to the conductors *a* and *b*, and constitutes the recharge observed.

1246. The following is the experiment on which I rest for the truth of this view.



Two plates of spermaceti,  $d$  and  $f$  (fig. 11.), were put together to form the dielectric,  $a$  and  $b$  being the metallic coatings of this compound plate, as before. The system was charged, then discharged, insulated, examined, and found to give no indications of electricity to the carrier ball. The plates  $d$  and  $f$  were then separated from each other, and instantly  $a$  with  $d$  was found in a positive state, and  $b$  with  $f$  in a negative state, nearly all the electricity being in the linings  $a$  and  $b$ . Hence it is clear that, of the forces sought for, the positive was in one half of the compound plate and the negative in the other half; for when removed bodily with the plates from each other's inductive influence, they appeared in separate places, and resumed of necessity their power of acting by induction on the electricity of surrounding bodies. Had the effect depended upon a peculiar relation of the contiguous particles of matter only, then each half plate,  $d$  and  $f$ , should have shown positive force on one surface and negative on the other.

1247. Thus it would appear that the best solid insulators, such as shell-lac, glass, and sulphur, have conductive properties to such an extent, that electricity can penetrate them bodily, though always subject to the overruling condition of induction (1178.). As to the depth to which the forces penetrate in this form of charge of the particles, theoretically, it should be throughout the mass, for what the charge of the metal does for the portion of dielectric next to it, should be done by the charged dielectric for the portion next beyond it again; but probably in the best insulators the sensible charge is to a very small depth only in the dielectric, for otherwise more would disappear in the first instance whilst the original charge is sustained, less time would be required for the assumption of the particular state, and more electricity would re-appear as return charge.

1248. The condition of *time* required for this penetration of the charge is important, both as respects the general relation of the cases to conduction, and also the removal of an objection that might otherwise properly be raised to certain results respecting specific inductive capacities, hereafter to be given (1269. 1277.).

1249. It is the assumption for a time of this charged state of the glass between the coatings in the Leyden jar, which gives origin to a well-known phenomenon, usually referred to the diffusion of electricity over the uncoated portion of the glass, namely, the *residual charge*. The extent of charge which can spontaneously be recovered by a large battery, after perfect uninsulation of both surfaces, is very considerable, and by far the largest portion of this is due to the return of electricity in the manner described. A plate of shell-lac six inches square, and half an inch thick, or a similar plate of spermaceti an inch thick, being coated on the sides with tin foil as a Leyden arrangement, will show this effect exceedingly well.

1250. The peculiar condition of dielectrics which has now been described, is evidently capable of producing an effect interfering with the results and conclusions drawn from the use of the two inductive apparatus, when shell-lac, glass, &c. is used in one or both of them (1192. 1207.): for upon dividing the charge in such cases ac-

according to the method described (1198. 1207.), it is evident that the one just receiving its half charge must fall faster in its tension than the other. For suppose app. i. first charged, and app. ii. used to divide with it; though both may actually lose alike, yet app. i., which has been diminished one half, will be sustained by a certain degree of return action or charge (1234.), whilst app. ii. will sink the more rapidly from the coming on of the particular state. I have endeavoured to avoid this interference by performing the whole process of comparison as quickly as possible, and taking the force of app. ii. immediately after the division, before any sensible diminution of the tension arising from the assumption of the peculiar state could be produced; and I have assumed that as about three minutes pass between the first charge of app. i. and the division, and three minutes between the division and discharge, when the force of the non-transferable electricity is measured, the contrary tendencies for those periods would keep that apparatus in a moderately steady and uniform condition for the latter portion of time.

1251. The particular action described occurs in the shell-lac of the stems, as well as in the *dielectric* used within the apparatus. It therefore constitutes a cause by which the outside of the stems may in some operations become charged with electricity, independent of the action of dust or carrying particles (1203.).

¶ v. *On specific induction, or specific inductive capacity.*

1252. I now proceed to examine the great question of specific inductive capacity, i. e. whether different dielectric bodies actually do possess any influence over the degree of induction which takes place through them. If any such difference should exist, it appeared to me not only of high importance in the further comprehension of the laws and results of induction, but an additional and very powerful argument for the theory I have ventured to put forth, that the whole depends upon a molecular action, in contradistinction to one at sensible distances.

The question may be stated thus: suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A at equal distances and uninsulated; A will then induce equally towards B and C. If in this position of the plates some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same? Will the relation of C and B to A be unaltered, notwithstanding the difference of the dielectrics interposed between them?

1253. As far as I recollect, it is assumed that no change will occur under such variation of circumstances, and that the relations of B and C to A depend entirely upon their distance. I only remember one experimental illustration of the question, and that is by COULOMB\*, in which he shows that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body as the same wire in air. The experiment offered to me no proof of the truth of the supposition, for it is not

\* Mémoires de l'Académie, 1787, pp. 452, 453.



the mere films of dielectric substances surrounding the charged body which have to be examined and compared, but the *whole mass* between that body and the surrounding conductors at which the induction terminates. Charge depends upon induction (1171. 1178.); and if induction relate to the particles of the surrounding dielectric, then it relates to *all* the particles of that dielectric inclosed by the surrounding conductors, and not merely to the few situated next to the charged body. Whether the difference I sought for existed or not, I soon found reason to doubt the conclusion that might be drawn from COULOMB'S result; and therefore had the apparatus made, which, with its use, has been already described (1187, &c.), and which appears to me well suited for the investigation of the question.

1254. Glass, and many bodies which might at first be considered as very fit to test the principle, proved exceedingly unfit for that purpose. Glass, principally in consequence of the alkali it contains, however well warmed and dried it may be, has a certain degree of conducting power upon its surface, dependent upon the moisture of the atmosphere, which renders it unfit for a test experiment. Resin, wax, naphtha, oil of turpentine, and many other substances were in turn rejected, because of a slight degree of conducting power possessed by them; and ultimately shell-lac and sulphur were chosen, after many experiments, as the dielectrics best fitted for the investigation. No difficulty can arise in perceiving how the possession of a feeble degree of conducting power tends to make a body produce effects, which would seem to indicate that it had a greater capability of allowing induction through it than another body perfect in its insulation. This source of error has been the one I have found most difficult to obviate in the proving experiments.

1255. *Induction through shell-lac.*—As a preparatory experiment, I first ascertained generally that when a part of the surface of a thick plate of shell-lac was excited or charged, there was no sensible difference in the character of the induction sustained by that charged part, whether exerted through the air in the one direction, or through the shell-lac of the plate in the other; provided the second surface of the plate had not, by contact with conductors, the action of dust, or any other means, become charged (1203.). Its solid condition enabled it to retain the excited particles in a permanent position, but that appeared to be all; for these particles acted just as freely through the shell-lac on one side as through the air on the other. The same general experiment was made by attaching a disc of tin foil to one side of the shell-lac plate, and electrifying it, and the results were the same. Scarcely any other solid substance than shell-lac and sulphur, and no liquid substance that I have tried, will bear this examination. Glass in its ordinary state utterly fails; yet it was essentially necessary to obtain this prior degree of perfection in the dielectric used, before any further progress could be made in the principal investigation.

1256. *Shell-lac and air* were compared in the first place. For this purpose a thick hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus (1187, &c.), so as nearly to fill the lower half of the space *o, o*



(fig. 1.) between it and the inner ball; and then charges were divided in the manner already described (1198. 1207.), each apparatus being used in turn to receive the first charge before its division by the other. As the apparatus were known to have equal inductive power when air was in both (1209. 1211.), any differences resulting from the introduction of the shell-lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point sought to be sustained. I have already referred to the precautions necessary in making the experiments (1199, &c.); and with respect to the error which might be introduced by the assumption of the peculiar state, it was guarded against, as far as possible, in the first place, by operating quickly (1248.), and afterwards by using that dielectric as glass or sulphur, which assumed the peculiar state most slowly, and in the least degree (1239. 1241.).

1257. The shell-lac hemisphere was put into app. i., and app. ii. left filled with air. The results of an experiment in which the charge through air was divided and reduced by the shell-lac app. were as follows:

App. i. Lac.	App. ii. Air.
Balls 255°.	
0° . . . .	
. . . .	304°
. . . .	297
Charge divided.	
113 . . . .	
. . . .	121
0 . . . .	after being discharged.
. . . .	7 after being discharged.

1258. Here 297°, minus 7°, or 290°, may be taken as the divisible charge of app. ii. (the 7° being fixed stem action (1203. 1232.)), of which 145° is the half. The lac app. i. gave 113° as the power or tension it had acquired after division; and the air app. ii. gave 121°, minus 7°, or 114°, as the force it possessed from what it retained of the divisible charge of 290°. These two numbers should evidently be alike, and they are very nearly so, far indeed within the errors of experiment and observation. But these numbers differ very much from 145°, or the force which the half charge would have had if app. i. had contained air instead of shell-lac; and it appears that whilst in the division the induction through the air has lost 176° of force, that through the lac has only gained 113°.

1259. If this difference be assumed as depending entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then this capacity for electric induction would be inversely as the respective loss and gain indicated above; and assuming the capacity of the air apparatus as 1, that of the shell-lac apparatus would be  $\frac{176}{113}$ , or 1.55.

1260. This extraordinary difference was so unexpected in its amount, as to excite the greatest suspicion of the general accuracy of the experiment, though the perfect discharge of app. i. after the division showed that the  $113^\circ$  had been taken and given up readily. It was evident that, if it really existed, it ought to produce corresponding effects in the reverse order; and that when induction through shell-lac was converted into induction through air, the force or tension of the whole ought to be *increased*. The app. i. was therefore charged in the first place, and its force divided with app. ii. The following were the results:

App. i. Lac.	App. ii. Air.
. . . .	$0^\circ$
$215^\circ$ . . . .	
$204$ . . . .	
Charge divided.	
. . . .	$118$
$118$ . . . .	
. . . .	$0$ after being discharged.
$0$ . . . .	after being discharged.

1261. Here  $204^\circ$  must be the utmost of the divisible charge. The app. i. and ii. present  $118^\circ$  as their respective forces; both now much *above* the half of the first force, or  $102^\circ$ , whereas in the former case they were below it. The lac app. i. has lost only  $86^\circ$ , yet it has given to the air app. ii.  $118^\circ$ , so that the lac still appears much to surpass the air, the capacity of the lac app. i. to the air app. ii. being as  $1.37$  to  $1$ .

1262. The difference of  $1.55$  and  $1.37$  as the expression of the capacity for the induction of shell-lac seems considerable, but is in reality very admissible under the circumstances, for both are in error in *contrary directions*. Thus in the last experiment the charge fell from  $215^\circ$  to  $204^\circ$  by the joint effects of dissipation and absorption (1192. 1250.), during the time which elapsed in the electrometer operations, between the applications of the carrier ball required to give those two results. Nearly an equal time must have elapsed between the application of the carrier which gave the  $204^\circ$  result, and the division of the charge between the two apparatus; and as the fall in force progressively decreases in amount (1192.), if in this case it be taken at  $6^\circ$  only, it will reduce the whole transferable charge at the time of division to  $198^\circ$  instead of  $204^\circ$ ; this diminishes the loss of the shell-lac charge to  $80^\circ$  instead of  $86^\circ$ ; and then the expression of specific capacity for it is increased, and, instead of  $1.37$ , is  $1.47$  times that of air.

1263. Applying the same correction to the former experiment in which air was *first* charged, the result is of the *contrary* kind. No shell-lac hemisphere was then in the apparatus, and therefore the loss would principally be from dissipation, and not from absorption: hence it would be nearer to the degree of loss shown by the numbers  $304^\circ$  and  $297^\circ$ , and being assumed as  $6^\circ$  would reduce the divisible charge to  $284^\circ$ . In

that case the air would have lost  $170^\circ$ , and communicated only  $113^\circ$  to the shell-lac; and the relative specific capacity of the latter would appear to be 1.50, which is very little indeed removed from 1.47, the expression given by the second experiment when corrected in the same way.

1264. The shell-lac was then removed from app. i. and put into app. ii. and the experiments of division again made. I give the results, because I think the importance of the point justifies and even requires them.

App. i. Air.	App. ii. Lac.
Balls $200^\circ$ .	
. . . .	$0^\circ$
$286^\circ$ . . . .	
283 . . . .	
Charge divided.	
. . . .	110
109 . . . .	
. . . .	$0.25$ after discharge.
Trace . . . .	after discharge.

Here app. i. retained  $109^\circ$ , having lost  $174^\circ$  in communicating  $110^\circ$  to app. ii.; and the capacity of the air app. is to the lac app., therefore, as 1 to 1.58. If the divided charge be corrected for an assumed loss of only  $3^\circ$ , being the amount of previous loss in the same time, it will make the capacity of the shell-lac app. 1.55 only.

1265. Then app. ii. was charged, and the charge divided thus:

App. i. Air.	App. ii. Lac.
$0^\circ$ . . . .	
. . . .	$256^\circ$
. . . .	251
Charge divided.	
146 . . . .	
. . . .	149
a little . . . .	after discharge.
. . . .	a little after discharge.

Here app. i. acquired a charge of  $146^\circ$ , while app. ii. lost only  $102^\circ$  in communicating that amount of force; the capacities being, therefore, to each other as 1 to 1.43. If the whole transferable charge be corrected for a loss of  $4^\circ$  previous to division, it gives the expression of 1.49 for the capacity of the shell-lac apparatus.

1266. These four expressions of 1.47, 1.50, 1.55, and 1.49 for the power of the shell-lac apparatus, through the different variations of the experiment, are very near to each other; the average is close upon 1.5, which may hereafter be used as the expression of the result. It is a very important result; and, showing for this parti-



cular piece of shell-lac a decided superiority over air in allowing or causing the act of induction, it proved the growing necessity of a more close and rigid examination of the whole question.

1267. The shell-lac was of the best quality, and had been carefully selected and cleaned; but as the action of any conducting particles in it would tend, virtually, to diminish the quantity or thickness of the dielectric used, and produce effects as if the two inducing surfaces of the conductors in that apparatus were nearer together than in the one with air only, I prepared another shell-lac hemisphere, of which the material had been dissolved in strong spirit of wine, the solution filtered, and then carefully evaporated. This is not an easy operation, for it is difficult to drive off the last portions of alcohol without injuring the lac by the heat applied; and unless they be dissipated, the substance left conducts too well to be used in these experiments. I prepared two hemispheres this way, one of them unexceptionable; and with it I repeated the former experiments with all precautions. The results were exactly of the same kind; the following expressions for the capacity of the shell-lac apparatus, whether it were app. i. or ii., being given directly by the experiments 1.46, 1.50, 1.52, 1.51; the average of these and several others being very nearly 1.5.

1268. As a final check upon the general conclusion, I then actually brought the surfaces of the air apparatus, corresponding to the place of the shell-lac in its apparatus, nearer together, by putting a metallic lining into the lower hemisphere of the one not containing the lac (1213.). The distance of the metal surface from the carrier ball was in this way diminished from 0.62 of an inch to 0.435 of an inch, whilst the interval occupied by the lac in the other apparatus remained 0.62 of an inch as before. Notwithstanding this change, the lac apparatus showed its former superiority; and whether it or the air apparatus was charged first, the capacity of the lac apparatus to the air apparatus was by the experimental results as 1.45 to 1.

1269. From all the experiments I have made, and their constant results, I cannot resist the conclusion that shell-lac does exhibit a case of *specific inductive capacity*. I have tried to check the trials in every way, and if not remove, at least estimate, every source of error. That the final result is not due to common conduction is shown by the capability of the apparatus to retain the communicated charge; that it is not due to the conductive power of inclosed small particles, by which they could acquire a polarized condition as conductors, is shown by the effects of the shell-lac purified by alcohol; and, that it is not due to any influence of the charged state, formerly described (1250.), first absorbing and then evolving electricity, is indicated by the *instantaneous* assumption and discharge of those portions of the power which are concerned in the phenomena, that effect occurring in these cases, as in all others of ordinary induction by charged conductors. The latter argument is the more striking in the case where the air apparatus is employed to divide the charge with the lac apparatus, for it obtains its portion of electricity in an *instant*, and yet is charged far above the *mean*.

1270. Admitting for the present the general fact sought to be proved; then 1·5, though it expresses the capacity of the apparatus containing the hemisphere of shell-lac, by no means expresses the relation of lac to air. The lac only occupies one half of the space  $o, o$ , of the apparatus containing it, through which the induction is sustained; the rest is filled with air, as in the other apparatus; and if the effect of the two upper halves of the globes be abstracted, then the comparison of the shell-lac powers in the lower half of the one, with the power of the air in the lower half of the other, will be as 2 : 1; and even this must be less than the truth, for the induction of the upper part of the apparatus, i. e. of the wire and ball B (fig. 1.) to external objects, must be the same in both, and considerably diminish the difference dependent upon, and really producible by, the influence of the shell-lac within.

1271. *Glass*.—I next worked with glass as the dielectric. It involved the possibility of conduction on its surface, but it excluded the idea of conducting particles within its substance (1267.) other than those of its own mass. Besides this it does not assume the charged state (1239.) so readily, or to such an extent as shell-lac.

1272. A thin hemispherical cup of glass being made hot was covered with a coat of shell-lac dissolved in alcohol, and after being dried for many hours in a hot place, was put into the apparatus and experimented with. It exhibited effects so slight, that, though they were in the direction indicating a superiority of glass over air, they were allowed to pass as possible errors of experiment; and the glass was considered as producing no sensible effect.

1273. I then procured a thick flint glass hemispherical cup resembling that of shell-lac (1239.), but not filling up the space  $o, o$ , so well. Its average thickness was 0·4 of an inch, there being an additional thickness of air, averaging 0·22 of an inch, to make up the whole space of 0·62 of an inch between the inducing metallic surfaces. It was covered with a film of shell-lac as the former was, (1272.) and being made very warm, was introduced into the apparatus, also warmed, and experiments made with it as in the former instances (1257. &c.). The general results were the same as with shell-lac, i. e. glass surpassed air in its power of favouring induction through it. The two best results as respected the state of the apparatus for retention of charge, &c., gave, when the air apparatus was charged first 1·336, and when the glass apparatus was charged first 1·45, as the specific inductive capacity for glass, both being without correction. The average of nine results, four with the glass apparatus first charged, and five with the air apparatus first charged, gave 1·38 as the power of the glass apparatus; 1·22 and 1·46 being the minimum and maximum numbers with all the errors of experiment upon them. In all the experiments the glass apparatus took up its inductive charge instantly, and lost it as readily; and during the short time of each experiment, acquired the peculiar state in a small degree only, so that the influence of this state, and also of conduction upon the results, must have been small.

1274. Allowing specific inductive capacity to be proved and active in this case, and



1·38 as the expression for the glass apparatus, then the specific inductive capacity of flint glass will be above 1·76, not forgetting that this expression is for a piece of glass of such thickness as to occupy not quite two-thirds of the space through which the induction is sustained (1273. 1253.).

1275. *Sulphur*.—The same hemisphere of this substance was used in app. ii. as was formerly referred to (1242.). The experiments were well made, i. e. the sulphur itself was free from charge both before and after each experiment, and no action from the stem appeared (1203. 1232.), so that no correction was required on that score. The following are the results when the air apparatus was first charged and divided :

App. i. Air.	App. ii. Sulphur.
Balls 280°.	
0° . . . .	
. . . .	0°
438 . . . .	
434 . . . .	
Charge divided.	
. . . .	162
164 . . . .	
. . . .	160
162 . . . .	
. . . .	0 after discharge.
0 . . . .	after discharge.

Here app. i. retained 164°, having lost 270° in communicating 162° to app. ii., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·66.

1276. Then the sulphur apparatus was charged first, thus :

. . . .	0°
0° . . . .	
. . . .	395
. . . .	388
Charge divided.	
237 . . . .	
. . . .	238
0 . . . .	after discharge.
. . . .	0 after discharge.

Here app. ii. retained 238°, and gave up 150° in communicating a charge of 237° to app. i., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·58. These results are very near to each other, and we may take the mean 1·62 as representing the specific inductive capacity of the sulphur apparatus; in which



case the specific inductive capacity of sulphur itself as compared to air = 1 (1270.) will be about or above 2·24.

1277. This result with sulphur I consider as one of the most unexceptionable. The substance when fused was perfectly clear, pellucid, and free from particles of dirt (1267.), so that no interference of small conducting particles confused the result. The body when solid is an excellent insulator, and by experiment was found to take up, with great slowness, that state (1241. 1242.) which alone seemed likely to disturb the conclusion. The experiments themselves, also, were free from any need of correction. Yet notwithstanding these circumstances, so favourable to the exclusion of error, the result is a higher specific inductive capacity for sulphur than for any other body as yet tried; and though this may in part be due to the sulphur being in a better shape, i. e. filling up more completely the space *o, o*, (fig. 1.) than the cups of shell-lac and glass, still I feel satisfied that the experiments altogether fully prove the existence of a difference between dielectrics as to their power of favouring an inductive action through them; which difference may, for the present, be expressed by the term *specific inductive capacity*.

1278. Having thus established the point in the most favourable cases that I could anticipate, I proceeded to examine other bodies amongst solids, liquids, and gases. These results I shall give with all convenient brevity.

1279. *Spermaceti*.—A good hemisphere of spermaceti being tried as to conducting power whilst its two surfaces were still in contact with the tin foil moulds used in forming it, was found to conduct sensibly even whilst warm. On removing it from the moulds and using it in one of the apparatus, it gave results indicating a specific inductive capacity between 1·3 and 1·6 for the apparatus containing it. But as the only mode of operation was to charge the air apparatus, and then after a quick contact with the spermaceti apparatus, ascertain what was left in the former (1281.), no great confidence can be placed in the results. They are not in opposition to the general conclusion, but cannot be brought forward as argument in favour of it.

1280. I endeavoured to find some liquids which would insulate well, and could be obtained in sufficient quantity for these experiments. Oil of turpentine, native naphtha rectified, and the condensed oil gas fluid, appeared by common experiments to promise best as to insulation. Being left in contact with fused carbonate of potassa, chloride of lime, and quick lime for some days and then filtered, they were found much injured in insulating power; but after distillation acquired their best state, though even then they proved to be conductors when large metallic contact was made with them.

1281. *Oil of turpentine rectified*.—I filled the lower half of app. i. with the fluid; and as it would not hold a charge sufficiently to enable me first to measure and then divide it, I charged app. ii. containing air, and dividing its charge with app. i. by a quick contact, measured that remaining in app. ii. : for, theoretically, if a quick contact would divide up to equal tension between the two apparatus, yet without sen-

sible loss from the conducting power of app. i.; and app. ii. were left charged to a degree of tension above half the original charge, it would indicate that oil of turpentine had less specific inductive capacity than air; or, if left charged below that mean state of tension, it would imply that the fluid had the greater inductive capacity. In an experiment of this kind, app. ii. gave as its charge  $390^\circ$  before division with app. i., and  $175^\circ$  afterwards, which is less than the half of  $390^\circ$ . Again, being at  $175^\circ$  before division, it was  $79^\circ$  after, which is also less than half the divided charge. Being at  $79^\circ$ , it was a third time divided, and then fell to  $36^\circ$ , less than the half of  $79^\circ$ . Such are the best results I could obtain; they are not inconsistent with the belief that oil of turpentine has a greater specific capacity than air, but they do not prove the fact, since the disappearance of more than half the charge may be due to the conducting power merely of the fluid.

1282. *Naphtha*.—This liquid gave results similar in their nature and direction to those with oil of turpentine.

1283. A most interesting class of substances, in relation to specific inductive capacity, now came under review, namely, the gases or aeriform bodies. These are so peculiarly constituted, and are bound together by so many striking physical and chemical relations, that I expected some remarkable results from them: air in various states was selected for the first experiments.

1284. *Air, rare and dense*.—Some experiments of division (1208.) seemed to show that dense and rare air were alike in the property under examination. A simple and better process was to attach one of the apparatus to an air pump, to charge it, and then examine the tension of the charge when the air within was more or less rarefied. Under these circumstances it was found, that commencing with a certain charge, that charge did not change in its tension or force as the air was rarefied, until the rarefaction was such that *discharge* across the space *o, o* (fig. 1.) occurred. This discharge was proportionate to the rarefaction; but having taken place, and lowered the tension to a certain degree, that degree was not at all affected by restoring the pressure and density of the air to their first quantities.

	inches of mercury.		
Thus at a pressure of . . . . .	30	the charge was . . . . .	$88^\circ$
Again . . . . .	30	the charge was . . . . .	88
Again . . . . .	30	the charge was . . . . .	87
Reduced to . . . . .	14	the charge was . . . . .	87
Raised again to . . . . .	30	the charge was . . . . .	86
Being now reduced to . . . . .	3.4	the charge fell to . . . . .	81
Raised again to . . . . .	30	the charge was still . . . . .	81

1285. The charges were low in these experiments, first that they might not pass off at low pressure, and next that little loss by dissipation might occur. I now reduced them still lower, that I might rarefy further, and for this purpose in the following experiment used a measuring interval in the electrometer of only  $15^\circ$  (1185.). The pressure of air within the apparatus being reduced to 1.9 inches of mercury,



the charge was found to be  $29^{\circ}$ ; then letting in air till the pressure was 30 inches, the charge was still  $29^{\circ}$ .

1286. These experiments were repeated with pure oxygen with the same consequences.

1287. This result of *no variation* in the electric tension being produced by variation in the density or pressure of the air, agrees perfectly with those obtained by Mr. HARRIS\*, and described in his beautiful and important investigations contained in the Philosophical Transactions; namely that induction is the same in rare and dense air, and that the divergence of an electrometer under such variations of the air continues the same, provided no electricity pass away from it. The effect is one entirely independent of that power which dense air has of causing a higher charge to be retained upon the surface of conductors in it than can be retained by the same conductors in rare air; a point I propose considering hereafter.

1288. I then compared *hot and cold air* together, by raising the temperature of one of the inductive apparatus as high as it could be without injury, and then dividing charges between it and the other apparatus containing cold air. The temperatures were about  $50^{\circ}$  and  $200^{\circ}$ . Still the power or capacity appeared to be unchanged; and when I endeavoured to vary the experiment, by charging a cold apparatus and then warming it by a spirit lamp, I could obtain no proof that the inductive capacity underwent any alteration.

1289. I compared *damp and dry air* together, but could find no difference in the results.

1290. *Gases*.—A very long series of experiments was then undertaken for the purpose of comparing *different gases* one with another. They were all found to insulate well, except such as acted on the shell-lac of the supporting stem; these were chlorine, ammonia, and muriatic acid. They were all dried by appropriate means before being introduced into the apparatus. It would have been sufficient to have compared each with air; but, in consequence of the striking result which came out, namely, that *all had the same power of, or capacity for, sustaining induction through them*, (which perhaps might have been expected after it was found that no variation of density or pressure produced any effect,) I was induced to compare them, experimentally, two and two in various ways, that no difference might escape me, and that the sameness of result might stand in full opposition to the contrast of property, composition, and condition which the gases themselves presented.

1291. The experiments were made upon the following pairs of gases.

1. Nitrogen and . . . Oxygen.
2. Oxygen . . . . Air.
3. Hydrogen . . . . Air.
4. Muriatic acid gas . Air.
5. Oxygen . . . . Hydrogen.
6. Oxygen . . . . Carbonic acid.

\* Philosophical Transactions, 1834, pp. 223, 224, 237, 244.



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|------------------------|-------------------------|
| 7. Oxygen . . . .      | Olefiant gas.           |
| 8. Oxygen . . . .      | Nitrous gas.            |
| 9. Oxygen . . . .      | Sulphurous acid.        |
| 10. Oxygen . . . .     | Ammonia.                |
| 11. Hydrogen. . . .    | Carbonic acid.          |
| 12. Hydrogen. . . .    | Olefiant gas.           |
| 13. Hydrogen. . . .    | Sulphurous acid.        |
| 14. Hydrogen. . . .    | Fluo-silicic acid.      |
| 15. Hydrogen. . . .    | Ammonia.                |
| 16. Hydrogen. . . .    | Arseniuretted hydrogen. |
| 17. Hydrogen. . . .    | Sulphuretted hydrogen.  |
| 18. Nitrogen . . . .   | Olefiant gas.           |
| 19. Nitrogen . . . .   | Nitrous gas.            |
| 20. Nitrogen . . . .   | Nitrous oxide.          |
| 21. Nitrogen . . . .   | Ammonia.                |
| 22. Carbonic oxide . . | Carbonic acid.          |
| 23. Carbonic oxide . . | Olefiant gas.           |
| 24. Nitrous oxide . .  | Nitrous gas.            |
| 25. Ammonia . . . .    | Sulphurous acid.        |

1292. Notwithstanding the striking contrasts of all kinds which these gases present of property, of density, whether simple or compound, anions or cations (665.), of high or low pressure (1284. 1286.), hot or cold (1288.), not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold between bodies having the gaseous condition and form. Another equally important electrical relation, which will be examined in the next paper, is that which the different gases have to each other at the *same pressure* of causing the retention of the *same or different degrees of charge* upon conductors in them. These two results appear to bear importantly upon the subject of electro-chemical excitation and decomposition; for as *all* these phenomena, different as they seem to be, must depend upon the electrical forces of the particles of matter, the very distance at which they seem to stand from each other will do much, if properly considered, to illustrate the principle by which they are held in one common bond, and subject, as they must be, to one common law.

1293. It is just possible that the gases may differ from each other in their specific inductive capacity, and yet by quantities so small as not to be distinguished in the apparatus I have used. It must be remembered, however, that in the gaseous experiments the gases occupy all the space *o, o*, (fig. 1.) between the inner and the outer ball, except the small portion filled by the stem; and the results, therefore, are twice as delicate as those with solid dielectrics.

1294. The insulation was good in all the experiments recorded, except Nos. 10, 15, 21, and 25, being those in which ammonia was compared with other gases. When shell-lac is put into ammoniacal gas its surface gradually acquires conducting power, and in this way the lac part of the stem within was so altered, that the ammonia apparatus could not retain a charge with sufficient steadiness to allow of division. In these experiments, therefore, the other apparatus was charged; its charge measured and divided with the ammonia apparatus by a quick contact, and what remained untaken away by the division again measured (1281.). It was so nearly one half of the original charge, as to authorize, with this reservation, the insertion of ammoniacal gas amongst the other gases, as having equal power with them.

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1295. Thus *induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount, but opposite in its direction and tendencies (1164.). Induction requires no sensible thickness in the conductors which may be used to limit its extent; an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states whilst the inductions continue. Nor is it affected by the nature of the limiting conductors, provided time be allowed, in the case of those which conduct slowly, for them to assume their final state (1170.).

1296. But with regard to the *dielectrics* or insulating media, matters are very different (1167.). Their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of *specific inductive capacities*, these differences being in some cases very great.

1297. The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines (1224.); or the attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsive or a diverging force in the transverse direction (1304.).

1298. Induction appears to consist in a certain polarized state of the particles, into which they are thrown by the electrified body sustaining the action, the particles assuming positive and negative points or parts, which are symmetrically arranged with respect to each other and the inducing surfaces or particles\*. The state must be a

\* The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognised matter. That is a question which I may be induced to consider in the next or following series of these researches.



forced one, for it is originated and sustained only by force, and sinks to the normal or quiescent state when that force is removed. It can be *continued* only in insulators by the same portion of electricity, because they only can retain this state of the particles (1304.).

1299. The principle of induction is of the utmost generality in electric action. It constitutes charge in every ordinary case, and probably in every case; it appears to be the cause of all excitement, and to precede every current. The degree to which the particles are affected in this their forced state, before discharge of one kind or another supervenes, appears to constitute what we call *intensity*.

1300. When a Leyden jar is *charged*, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. *Discharge* is the return of these particles to their natural state from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.

1301. All charge of conductors is on their surface, because being essentially inductive, it is there only that the medium capable of sustaining the necessary inductive state begins. If the conductors are hollow and contain air or any other dielectric, still no *charge* can appear upon that internal surface, because the dielectric there cannot assume the polarized state throughout, in consequence of the opposing actions in different directions.

1302. The known influence of *form* is perfectly consistent with the corpuscular view of induction set forth. An electrified cylinder is more affected by the influence of the surrounding conductors (which complete the condition of charge) at the ends than at the middle, because the ends are exposed to a greater sum of inductive forces than the middle; and a point is brought to a higher condition than a ball, because, by relation to the conductors around, more inductive force terminates on its surface than on an equal surface of the ball with which it is compared. Here too, especially, can be perceived the influence of the lateral or transverse force (1297.), which, being a power of the nature of or equivalent to repulsion, causes such a disposition of the lines of inductive force in their course across the dielectric, that they must accumulate upon the point, the end of the cylinder, or any projecting part.

1303. The influence of *distance* is also in harmony with the same view. There is perhaps no distance so great that induction cannot take place through it\*; but with the same constraining force (1298.) it takes place the more easily, according as the extent of dielectric through which it is exerted is lessened. And as it is assumed by the theory that the particles of the dielectric, though tending to remain in a normal state, are thrown into a forced condition during the induction; so it would seem to

\* I have traced it experimentally from a ball placed in the middle of the large cube formerly described (1173.) to the sides of the cube six feet distant, and also from the same ball placed in the middle of our large lecture-room to the walls of the room at twenty-six feet distance, the charge upon the ball in these cases being solely due to induction through these distances.



follow that the fewer there are of these intervening particles opposing their tendency to the assumption of the new state, the greater degree of change will they suffer, i. e. the higher will be the condition they assume, and the larger the amount of inductive action exerted through them.

1304. I have used the phrases *lines of inductive force* and *curved lines of force* (1231. 1297. 1298. 1302.) in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is of course the resultant of compound forces, every molecule being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous. The transverse force is merely this relation considered in a direction oblique to the lines of inductive force, and at present I mean no more than that by the phrase. With respect to the term *polarity* also, I mean at present only a disposition of force by which the same molecule acquires opposite powers on different parts. The particular way in which this disposition is made will come into consideration hereafter, and probably varies in different bodies, and so produces variety of electrical relation. All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate. Further inquiry, I trust, will enable us by degrees to restrict the sense more and more, and so render the explanation of electrical phenomena day by day more and more definite.

1305. As a test of the probable accuracy of my views, I have throughout this experimental examination compared them with the conclusions drawn by M. Poisson from his beautiful mathematical inquiries\*. I am quite unfit to form a judgment of these admirable papers; but as far as I can perceive, the theory I have set forth and the results I have obtained are not in opposition to such of those conclusions as represent the final disposition and state of the forces in the limited number of cases he has considered. His theory assumes a very different mode of action in induction to that which I have ventured to support, and would probably find its mathematical test in the endeavour to apply it to cases of induction in curved lines. To my feeling it is insufficient in its mode of accounting for the retention of electricity upon the surface of conductors by the pressure of the air, an effect which I hope to show is simple and consistent according to the present view; and it does not touch voltaic electricity, or in any way associate it and what is called ordinary electricity under one common principle.

I have also looked with some anxiety to the results which that indefatigable philosopher HARRIS has obtained in his investigation of the laws of induction†, knowing that they were experimental, and having a full conviction of their exactness; but I am happy in perceiving no collision at present between them and the views I have set forth.

1306. Finally, I beg to say that I put forth my particular view with doubt and fear,

\* Mémoires de l'Institut, 1811, tom. xii. the first page 1, and the second paging 163.

† Philosophical Transactions, 1834, p. 213.

lest it should not bear the test of general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with all known facts, and the manner in which it linked together effects apparently very different in kind, urged me to write the present paper. I as yet see no inconsistency between it and nature, but, on the contrary, think I perceive much new light thrown by it on her operations; and my next papers will be devoted to a review of the phenomena of conduction, electrolyzation, current, magnetism, retention, discharge, and some other points, with an application of the theory to these effects, and an examination of it by them.

*Royal Institution,*  
*November 16, 1837.*











